THE NATURE OF ESSENTIAL HYPERTENSION

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FOR many years it has been customary for clinicians to divide patients with high blood-pressure into two groups, essential hypertension and secondary. The list of recognised causes of secondary hypertension has naturally grown with advances in knowledge, and includes disorders of the kidney (probably acting through the production of renal ischæmia) and hypersecretion of the suprarenal cortex or medulla. Essential hypertension is usually looked upon not merely as the residue of the cases after primary causes have been sought without success, but as a syndrome which has recognisable features, the most important of which are that it does not usually become severe enough to produce symptoms until middle age, and that it has a strong hereditary background.

Investigating the first of these two features, Platt (1948) showed that below the age of 40, and especially below the age of 35, severe hypertension was far more often to be placed in the secondary than in the essential group, and investigating the second of these features, Platt (1947) showed that cases carefully diagnosed as secondary hypertension did not commonly show the family history suggestive of high blood-pressure which was found in the majority of cases of essential hypertension. Because essential hypertension has been known to run through three generations, and because in nearly every case one or both parents seemed to be affected, Platt postulated (but did not claim to prove) that the disease might be due to a gene behaving as a mendelian dominant. This paper, though having a number of defects, since it rested on a comparatively small number of cases, and upon the questionable reliability of family histories, nevertheless seemed to show for the first time a clear distinction between the heredity of essential and of secondary hypertension. Looked at from the point of view of human evolution it is likely that the two main features of essential hypertension—namely, that it does not seriously affect life expectancy until middle age, and that it has a hereditary background are interrelated, for a hereditary disease which affected survival during reproductive life would rapidly become a rarity in any population.

This concept c essential hypertension as an inherited tendency to develop high blood-pressure in middle life presumes that there are two populations, one in which blood-pressure rises significantly in middle age, often reaching heights at which it seriously contributes to mortality, and another population in which the blood-pressure rises only very little if at all with increasing years. If this is the case any sample of blood-pressures in the general population will show a rise with age, since it will in fact be a mean of the two populations. It would also be expected that the frequency distribution of blood-pressure in middle age would show a bimodal curve with one peak at a so-called normal level of blood-pressure and another peak representing the hypertensive members of the population. The tendency to hypertension, however, manifests itself to varying degrees, and is much influenced by environmental factors, and it is therefore likely that the two curves would overlap.

Essential Hypertension not an Entity ?

In a series of papers in 1954 Hamilton, Pickering, Fraser Roberts, and Sowry published further investigations into the ætiology of essential hypertension. Looking af the frequency distribution of blood-pressure at different ages they found clear evidence of blood-pressure rising with age, and in the older age-groups the curves tended to become broader and to depart considerably from the typical gaussian shape, showing in every case a bulge or positive skew deviation in the region of the higher pressures. Commenting on these curves they say : " While these distribution curves do not exclude the possibility that we are dealing with more than one kind of population as regards blood-pressure, there is nothing to suggest that this is so, or that there is any natural division between normal and abnormal. It would seem, in fact, that the term essential hypertension represents no more than that section of the population in which the arterial pressure exceeds an arbitrary value and in which no other disease is found to account for the arterial pressures observed. According to where we draw the line, so we can make essential hypertension as common or as rare as we wish ".

Examining the relatives (parents, siblings, and children) of patients with hypertension, these workers again found no evidence of a bimodal curve. Although with proper caution they admit, in the paragraph just quoted, that their curves could be differently interpreted, Pickering in a later publication (1955) has clearly adopted the thesis that essential hypertension does not exist as an entity, that blood-pressure is a continuous and not a discontinuously distributed feature of the population, and that what we call essential hypertension is no more than the tail end of a distribution curve. Children tend to resemble their parents with regard to blood-pressure as they do with regard to height, intelligence, and other continuously distributed characteristics.

This new concept of essential hypertension, attractive as it is in its simplicity, has failed to convince a number of clinicians who feel that it does not fit in with their experience, and who still think that the majority of middle-aged patients can be divided into those who have high blood-pressure and those who do not. This may be because clinicians are slow to adopt a new theory, because clinical impressions are notoriously inaccurate, or because the human mind likes to place things into categories, and especially likes to go on doing so if this is the established custom. On the other hand, it may be that a bimodal curve is concealed in the skew deviation already mentioned which clearly shows also in the data of Boe, Humerfelt, and Wederwang (1957).

There seem to be at least two possible factors which may tend to obscure the facts when populations rather than individuals are studied. First, if it be true that essential hypertension is a disorder which manifests itself chiefly between the ages of 45 and 60, there must be younger ages at which the blood-pressure is rising at different rates and to varying degrees in different indi-

viduals. Secondly, in considering the hereditary aspect of hypertension Hamilton et al. (1954) admit that they did not in all cases clearly distinguish between essential and secondary hypertension. Of the 110 hypertensive propositi listed in their fourth paper, 30 are aged 40 or less, an age at which severe essential hypertension is unusual, and 7 were aged 65 or more, an age at which senile or arteriosclerotic hypertension admitted by Pickering (1955) to be probably of different ætiology occurs.

The Data Re-examined

It seemed to me that there may be a critical way of re-examining the existing data which would help to dispose of the two complicating factors.

If essential hypertension is a disorder of middle age developing to a variable degree as the result of the inheritance of a major dominant gene or genes, it would be expected that the siblings of hypertensive patients would segregate into two groups, those who inherited and those who did not inherit the disorder from their parents. In order to exclude as far as possible cases of secondary hypertension and cases of senile hypertension, and to restrict the study to an age at which the gene, if inherited, would be expected to express itself, we ought to examine the siblings aged 45—60 of persons aged 45—60 with high blood-pressure. If essential hypertension exists as an entity we would expect a bimodal curve; if on the other hand blood-pressure is continuously distributed in the population there should be no such appearance. Restriction of the investigation to patients and to their siblings who will be of approximately the same mean age disposes of the necessity for making adjustments for age which could be another source of error, and should no obscure the evidence for a continuous distribution if such it is. Plotting the distribution curves for blood-pressure of siblings aged 45—60 of hypertensive subjects aged 45—60 from the data recorded by Pickering and his co-workers (figs 1 and 2) clearly shows a bimodal curve for systolic pressure, and probably also for diastolic pressure. In order to increase the number of cases, Pickering and his co-workers added to their own data the very useful data recorded Sobye (1948), after having shown that Sobye's criteria were reasonably similar to their own. Fig. 3 and 4 show the data of Sobye treated in the same way—namely, the distribution of systolic and diastolic pressures in siblings aged 45-60 of hypertensives aged 45-60. With these more extensive figures the bimodality of the curves clearly stands out. Figs. 5 and 6 show the data of Sobye and Pickering added together. The combined series comprise 252 siblings of hypertensives. No propositi are included.

It is clear from the data of Pickering and Sobye that people who take blood-pressures more often record their results in even tens than in fives, and in order to construct the curves we have therefore added diastolic pressures at 70 and 75, again at 80 and 85, and at 90 and 95, etc. If one tries to get over this difficulty in another way by adding 75 to 80, 85 to 90, etc., it makes the diastolic curve of the Pickering data smooth out but does not affect the rest. If one distributes the fives between the neighbouring tens all the curves remain bimodal. We do not think that such inaccuracies in recording can account for these rather striking curves, because they are not bimodal when all the pressures of all relatives at all ages are included. Moreover the curves show a clearer dip in the systolic than in the diastolic distribution

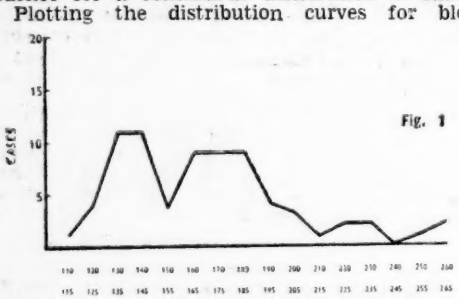

Fig. 1

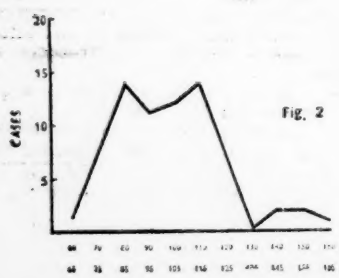

Fig. 2

Figs. 1 and 2. Frequency distribution of (1) systolic and (2) diastolic pressures of siblings aged 45 to 60 of hypertensives aged 45 to 60 from data of Hamilton, Pickering, Fraser Roberts, and Sowry.

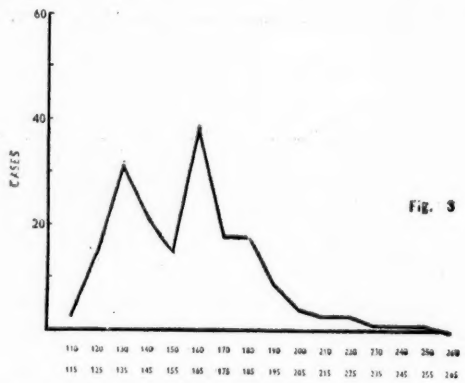

Fig. 3

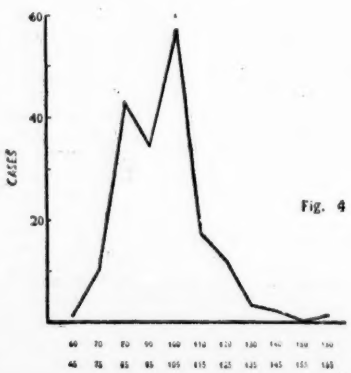

Fig. 4

Figs. 3 and 4. The same from data of Sobye.

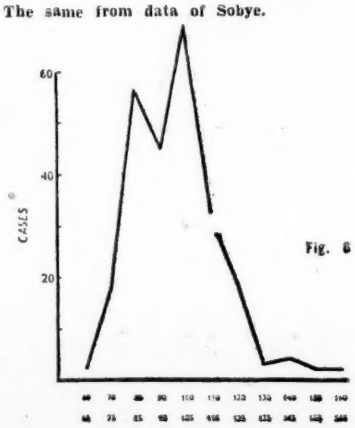

Fig. 5

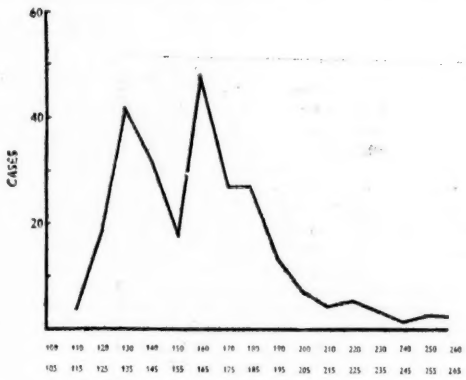

Fig. 5

Figs. 5 and 6. Data of figs. 1 and 3 and 2 and 4 combined.

curves. Even if there were a tendency for recorders to ignore pressures just below 100, there seems no reason to suppose that the same recorders would ignore pressures of around 150. It is interesting that the main dip in the curve occurs at about 150/90, which is exactly the figure most commonly chosen by clinicians as the dividing line between normal and high blood-pressure in middle-aged subjects.

With a characteristic as variable as blood-pressure, subject as it is to psychological and other environmental factors, one would not expect to be able to carry the proof of a dominant inheritance to a final conclusion, but it is perhaps interesting to note that if we use the systolic pressure of 150 as the dividing line, the proportion of hypertensive siblings is about 0.59. If, as many people hold, hypertension affects 20-30 % of the middle-aged population, giving a gene frequency for the recessive of 0.84 to 0,9, this about the proportion we should expect*.

The final proof as to whether essential hypertension exists as an entity will have to wait until a prospective survey lasting at least twenty years has been made over a large unselected population to see whether with advancing years they segregate into two groups those whose pressures steeply rise and those whose pressures remain little affected. Until this is done I shall remain unconvinced that the work of Pickering and his colleagues has disposed of the concept of the entity of essential hypertension.

Summary

This paper re-examines the data of Hamilton, Pickering, Fraser Roberts, and Sowry on essential hypertension and gives reasons for doubting their hypothesis as to its nature. It is quite short. The reasons cannot be summarised and those who seek to understand the arguments should thereforce read the paper.

*) Suppose that in middle age about 19-30 % (figures chosen) for convenience) of the population have essential hypertension. Then for a population of the composition $p2+2pq+q2$, the recessive non-hypertensive fraction, $q2=0.81$ or 0.70. Then $q=0.9$ or 0.84 (approximately), and $p=0.1$ or 0.16. The proportion of hypertensive siblings of hypertensive propositi should be

$$1-\frac{pq2\ (1-p/4)}{1-q2}=0.58 \text{ or } 0.64$$

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Preliminary Communications

" TEGO " COMPOUNDS IN HOSPITAL PRACTICE

THE " Tego " series [1, 2] of compound are amino acids of high molecular weight and are ampholytic surface-active bactericidal agents. They combine the detergent properties of anionic compounds with the bactericidal properties of the cationic compounds, and they remain highly active in the presence of protein. They are inactivated, however, by soap and synthetic detergents.

I have tested their efficacy for various purposes

PREOPERATIVE HAND CLEANSING

The compound used was " Tegolan " emulsion containing two active compounds : N Dodecyl-1, 3-amino-propyl-aminobutyric acid and O Dodecyl-1, 3 oxy-propyl-aminobutyric acid.

The hands were not wetted before using the emulsion. 2-3 ml. of tegolan was run into the palm of one hand, and then worked well over both hands, wrists, and forearms. From a running tap enough water was taken up to allow a copious lather to be produced. The hands were lathered for two minutes, and the nails scrubbed with a brush. The lather was rinsed off, and a further 2 ml. of tegolan taken into the palm. This was relathered for thirty seconds, after which the hands and arms rinsed again, and dried on a sterile towel.

The hands were swabbed before cleansing, using swabs soaked in nutrient broth. Immediately after the operation they were swabbed again by passing a similar swab down the glove along the palmar surface of the hand, and down a finger before the gloves were removed. None of the operations performed took less than an hour and most lasted well over this time.

The swabs were broken off into 15 ml. of Robertson's cooked-meat medium and incubated for twenty-four hours at 37°C. The were then plated out on two blood-agar plates one being incubated ærobically, and the other anærobically at 37° C.

Of the 51 swabs taken before cleansing the hands, 3 gave no growth and the remaining 49 all yielded micrococci, 2 yielded aerobic spore-bearing organisms, and six yielded. *Staphylcoccus aureus*. Of the 51 corresponding swabs taken from the hands immediately after the operations, 47 gave no growth of aerobic sporebearing organisms. These organisms were present in the corresponding swabs taken before the operation.

This method of hand cleansing offers three distinct advantages : *(a)* the time required for " scrubbing up " is much reduced ; *(b)* the need for scrubbing the skin is eliminated; and *(c)* the skin surface as a rule remains free from viable organisms for long periods.

PREOPERATIVE SKIN PREPARATION

For this series of tests tego 103S was used. This is an aqueous solution of dodecyl-di-(aminoethyl)-glycine hydrochloride.

The shaved and washed skin was prepared just before operation by being thoroughly swabbed twice with a 1 % solution of the compounds, and then left wet.

Of the 24 swabs taken from the skin before it was prepared 5 gaves no grown. The remaining 19 all gave a growth of micrococci. In addition, 4 also gave a growth of *Str. viridans*, a growth of *Bacterium anitratum*, and 1 a growth of aerobic spore-bearing organisms. Of the 24 corresponding swabs taken from the closed wound at the end of the operation 3 gave a growth of micrococci, and 21 gave no growth.

These findings indicate that tego 103S is a good skin-cleansing agent for preoperative use.

Before the wounds were dressed they were again swabbed with tego 103S and then covered with

1. Perkins, F. T., Short, D. J. *Hosp. Engineer*, 1957, 11, 270.
2. Schmitz, I. A., Harris, S. W. *M/g. Chem.* 1958, 39, 51.

"Nobecutane". All wounds were swabbed on the third postoperative day at the time of the first dressing. Of 33 wounds swabbed, 8 gave no growth, and of the 25 giving a growth, 3 produced *Staph. aureus* (but did not look "infected"), 17 yielded micrococci, 2 yielded *Str. viridans*, and 1 yielded *Escherichia coli* and *Bact. anitratum*.

Compared with the wound-infection rate for two other wards [3], this was an extremely good record.

CLEANSING OF OPERATING THEATRE FLOORS, WALLS, AND EQUIPMENT

A machine was used for this, and consists of the tego mixing apparatus and spray lance. This device is very easy to operate and the staff like it. It is filled with tego MHG (dodecyl-di-laminoethylglycine), and on being connected to a mains water-supply it automatically delivers a 1 % solution of the bactericidal agent of the form of a fine spray.

When two new theatres equipped with plenum air conditioning were completed for the Nuffield department of surgery, their floors, walls, and equipment were swabbed in March, 1958, using coton-wool swabs moistened with broth. Of the 20 swabs first taken, 19 gave no growth of the two pathogenic organisms looked for. *Staph. aureus* and *Clostridium welchii*. 1 swab from some wooden shelves in a preparation room gave a growth of *Cl. welchii*.

The theatre then went into use with the following cleansing routine. Once a week it was thoroughly cleansed with the tego spray lance, excess fluid being removed by mopping. During the week, and if any dirt had to be washed away, the floor and walls were mopped with 1 % tego MHG. Swabs were taken weekly on the day before the tego machine was used.

During April, 20 swabs were taken, all of which were negative for *Staph. aureus* and *Cl. welchii*. This satisfactory condition continued with fortnightly swabbing until November when the sample of tego MHG had all been used and the theatres were cleaned with 1 % carbolic acid. A fortnight later, of 32 swabs taken, 6 yielded *Cl. welchii*. A fortnight later, of 32 swabs taken, 17 were positive for *Cl. welchii*, and it was obvious that a dangerous increase in the numbers of *Cl. welchii* was occurring. A fresh supply of tego MHG was then obtained, and within a fortnight of its being used again the theatre appeared to be free from *Cl. welchii*.

It was discovered, however, that the surgeons' and nurses' changing-rooms and the nurses' sitting-room were rich sources of the organisms. Cleansing with tego was introduced, there, and within a fortnight none of the swabs taken gave a growth of *Cl. welchii* or *Staph. aureus*. The contamination was apparently being carried into the theatre on the boots of the staff. Of 24 boots tested before tego MHG was reintroduced, 20 gave a growth of *Cl. welchii*. Since the boots were washed in 1 % tego MHG, none has given a growth of the organism.

WARD CLEANSING

Two newly opened wards, L and H, were swabbed in January, 1958. Of the 66 swabs taken from L ward, 2 yielded *Staph. aureus*, and of the 52 from H ward, 1 yielded *Staph. aureus*.

The floors of the wards were of thermoplastic tiles. The floor of L ward was washed weekly with 1 % tego MHG, and then polished with water wax. The floor of H ward was washed weekly with plain soap, and polished in the same way. The blankets in use in both wards were of wool and were laundered during the period described. Swabs of the floors were taken weekly, on the day before the cleansing with tego MHG was done.

Of the 10 swabs taken from each of the two wards treated with tego, 3 from L and 7 from H gave a growth of *Staph. aureus*. When the tego sample was used up, the floors of both wards were washed with soap. Three weeks later, of the 14 swabs taken from L, 12 gave a growth of *Staph. aureus*, and of the 10 swabs from H ward, 2 gave *Staph. aureus*.

The value of tego in the wards will obviously be difficult to assess, but the above results justify further trials, particularly where the blankets are of coton and are being regularly and frequently cleansed with a bactericidal agent [3].

That the method of culturing the swabs was adequate for neutralising any residual tego was demonstrated by taking paired swabs from floors known to be contamined with *Staph. aureus* and *Cl. welchii* and dipping one swab of each pair in 1 % tego MHG immediately before culturing. All 6 pairs taken for *Cl. welchii* were positive, and 4 pairs of 6 taken for *Staph. aureus* were positive. Thus a quantity of tego MHG larger than that which could be picked up from a surface previously washed with tego MHG was neutralised by the culture media used.

I am indebted to Dr. F. T. Perkins for drawing my attention to "Tego" compounds; to Mr. Sim Harris, of Hough and Hoseason, Ltd, Atlas Laboratories, Levenshulme, Manchester, 19, for the gifts of Tego compounds; and to Prof. P. R. Allison and Mr. A. J. Gunning of the Nuffield Department of Surgery, Radcliffe Infirmary, Oxford, for their cooperation in these investigations.

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Reviews of Books

Mental Illness in London

VERA NORRIS, M.B., PH. D., late lecturer in medical statistics, Institute of Psychiatry, London. Maudsley Monographs no. 6. London: Chapman & Hall for the Institute of Psychiatry. 1959. Pp. 317. 35s.

THIS important monograph lives up to the reputation of its forerunners. Written by Dr. Norris shortly before her death, it is based on a mass of figures related to the admission and discharge of some 10,000 patients in the London area between 1947 and 1959: the thorough handling of the material and the careful conclusions drawn from it are what we are accustomed to from this series. Follow-up and rediagnosis showed that the broad diagnostic groupings were statistically fairly reliable, and the poor prognosis of schizophrenia appeared from the third that never left hospital after the first admission and the further third that returned within four years. Manic-depressive illness was rather less gloomy, the average stay being a year: half the cases were in hospital for over four months and 9 % for at least four and a half years. The total prognostic picture did not seem to bear out the therapeutic optimism of some advocates of the newer physical treatments. Single people required admission more often than married, and women more often than men, although more men were diagnosed as psychoneurotic or as having some disorder of behaviour or character. Compared with figures of last century, it appears that married women make up a higher proportion of admissions; and this was tentatively related to the emptiness of the life of the modern wife with her small family and loss of interesting employment after marriage. Expectancy rates for schizophrenia were confirmed at about 1 %—much the same figure as for manic-depressive illness, although in this disorder the female expectancy was nearly twice that of the male. As regards the hospital administration, Dr. Norris drew a number of interesting conclusions—notably that at a time when an increasing proportion of the population need admission to mental hospitals, the provisions in hand to cope with this need are paltry, both as to research and to accommodation and treatment.

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THE LANCET

LONDON 25 JULY 1959

Live Poliomyelitis Virus Vaccine

EVEN in the exciting times in which we live few medical controversies stir so many emotions as that between the proponents of killed and live poliomyelitis virus vaccines. First attempts by BRODIE [1] and KOLMER [2] to produce a killed virus vaccine in 1936 were a failure; and first efforts to produce a live virus vaccine by KOPROWSKI et al. [3] in 1952 were wisely not pursued too vigorously at that time. The development of poliomyelitis vaccines today owes its stimulus to the work of ENDERS, WELLER, and ROBBINS [4] who were able to cultivate the virus successfully in tissue culture and to devise elegant methods for titrating its activity. From this baseline SALK and coworkers [5] evolved the formolised virus vaccine which, after an uncertain start, is now in widespread use. With the present precautions this vaccine is safe. But it has drawbacks, notably that protection though high is by no means complete, that three or perhaps four injections may be needed to produce adequate protection, that vaccination may not prevent alimentary infection with virus which may then spread to unvaccinated individuals [6-7], and that vaccination is largely ineffective in halting a poliomyelitis epidemic.

SALK [8] argues that vaccination may nevertheless reduce virus transmission, which he believes may depend mainly on pharyngeal rather than fæcal excretion of virus. But he concludes that from the practical standpoint one should not rely upon this possibility and that vaccination implies a "sharing of community responsibility if it is reasonable to believe that the total reservoir of infection may be reduced by widespread individual vaccination". SALK also believes that durable immunity can be produced by three injections of a potent killed virus vaccine and BARON et al. [9] suggest that rapid onset of immunity can be achieved by a single inoculation of 10 ml. of vaccine. But although the modern killed virus vaccine was born first and is developing rapidly, its younger brother, live virus vaccine, shows no signs of allowing itself to be left behind. The proponents of live virus vaccine are agreed that this is the method of choice, but about the means of achieving this aim there are almost as many opinions as there are workers in the field. There is therefore a heavy responsibility on physicians, public-heath workers, and scientists to examine the evidence as dispassionately as they can.

A conference [10] was lately convened in Georgetown University by the World Health Organisation to evaluate live virus vaccines, with a view to using them in a worldwide programme to eradicate poliomyelitis. SABIN told the conference that his live virus vaccine had been given safely to 4½ million people, particularly in the Soviet Union, Czechoslovakia, Mexico, Poland, and Malaya. He was supported by M.P. CHUMAKOV, of Moscow, who said that 1½ million people had been vaccinated with SABIN's virus in the Soviet Union with results which indicated complete safety and high immunological efficiency. In reply to a plea for caution in view of the observation that an increase in virulence for the monkey can follow passage of the virus through man, SABIN is reported to have said that he has new evidence that a progressive increase in virulence does not follow continued passage of his vaccine strain in the human intestine. H.R. Cox produced a surprise when he announced the development of an oral trivalent live virus vaccine by Lederle Laboratories. It had been found previously that when two different serotypes were fed simultaneously one interfered with development of immunity to the other. Cox and others found that it was possible to maintain a mixed infection of two serotypes in tissue culture and further studies showed that it was possible to immunise against all three types of virus with one feeding. The advantages claimed for this method are a great saving in the cost of vaccine and its administration (important factors for underdeveloped countries) and full protection from one swallow a big claim at this stage. Nevertheless it has already been given safely to many infants and adults, including women in the last month of pregnancy. (This must presumably involve the risk of vaccinating the newly born infant at the same time.)

KOPROWSKI spoke on a theme he has recently discussed [12]—the fact that immunisation with live virus has given immune responses which have already lasted 9 years in the absence of any evidence of reinfection.

The idea of attempting to eradicate poliomyelitis is ambitious, and what evidence there is at the moment suggests it is likely to be achieved by a live vaccine rather than a killed one. But the choice for public-health authorities will be full of headaches. The Salk vaccine has less unknowns at the moment, and a natural inclination to favour the more familiar will discourage many from considering changes. But there is one situation in which Salk vaccine is useless and in which the advantages of live vaccine probably already outweigh its disadvantages and that is in the face of a large poliomyelitis epidemic. For this reason the recent study of HALE et al. [13] is likely to assume great importance. They vaccinated 200,000 children in Singapore with a live avirulent type-2 poliomyelitis virus, during the course of an epidemic caused by type-1 virus at the end of 1958. The reasons for choosing a type-2 virus were, firstly, that it should be possible to see by laboratory tests whether the vaccine was causing paralytic poliomyelitis in vaccinated individuals or their contacts. In fact there was no evidence that this was so, although the type-2 virus seems to have spread quite widely to

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unvaccinated children. Secondly, a type-2 virus was chosen in the hope that it might interfere with the establishment of type-1 infection, as has been shown experimentally in chimpanzees [14]. Thirdly, there was also a possibility of producing some specific immunity since second paralytic attacks of poliomyelitis are rare. The protection was difficult to estimate since vaccination lasted a few weeks, during which a proportion of theoretically non-vaccinated individuals became accidentally vaccinated from contact with vaccinated children. The degree of protection may therefore have been underestimated. There were 6 cases of paralytic poliomyelitis due to the type-1 strain among some 200,000 vaccinated children, excluding children who were incubating type-1 infection when they were given the vaccine. During the same period from the first week after the beginning of the vaccination campaign there were 179 paralytic type-1 cases in about 300,000 non-vaccinated children of the same age-group as the vaccines. On these figures, the vaccine has protected at least 95 % of those vaccinated over one week.

These figures are sufficiently striking to demand further investigation. There is at least an a-priori case for repeating investigations of this kind where there is no other known method of halting a poliomyelitis epidemic. HALE et al. also point out that the reported increase in virulence for monkeys after administration of avirulent virus to man is a laboratory measurement which may not apply in the field. While there may be an element of wishful thinking in this argument the figures of protection speak for themselves. There is a need to use trials of this kind not only to assess the protective value of different live virus vaccines but also to gain as much basic information as possible on the virulence and spread of different viruses and their ultimate protective effects. The pace of this work is quickening but we hope that live vaccines will not be asked to do too much before they are ready.

14. Sabin, A.B. *J. Amer. med. Ass.* 1956, **162**, 1589.

Annotations

THE EDINBURGH MEETING

FOR the past ten days the British Medical Association has been meeting at Edinburgh. The first four of these were devoted to the representative meeting, and this week 2000 Canadian doctors joined their British colleagues for the long-awaited joint scientific programme.

The representative body rejected by a large majority Sir Frank Newsam's advice [1] that withdrawal of doctors from the National Health Service should no longer be considered even as a last resort. Nevertheless many doubted the feasibility of such withdrawal. Even neighbouring divisions took quite different views on whether an alternative scheme would work and how far it would alienate public opinion. Some felt that no Government would tolerate the threat to its authority which withdrawal would mean. Others said that the Government's refusal to submit to arbitration had placed doctors in a strong moral position. Others again thought that it would be foolish to forfeit this moral advantage, as might happen if doctors withdrew from the service on grounds of salary alone. The council had reported on the principles on which they thought a possible alternative service might be based, and the conference agreed to regard this as a progress report.

Later in the meeting discussion centred on possible intrusion of the hospital into general practice, which would reduce the status and prestige of the family doctor, so that—in one delegate's words—he might even become a "mere houseman" in his own practice. Nevertheless, those with experience of one such innovation (namely, child care by a hospital paediatric team [2]) could not praise it too highly, and had never found their authority lessened. To some the Cranbrook report on maternity services [3] seemed to be a similar threat to independence in general practice, and the conference passed a series of resolutions criticising those sections of the report which attempted to lay down qualifications for inclusion on the "obstetric list". Several speakers pointed out that it was the first time that a Government department had challenged the prerogative of the General Medical Council to lay down standards of professional competence. If there was to be an "obstetric list", might there not later be "paediatric" and "medical" lists, exclusion from which would debar practitioners from working in these branches of medicine?

1. Newsam, F.A. *Brit med. J.* (suppl.) 1959, i, 12.
2. Lightwood, R., Brimblecombe, F.S.W., Rheinhold, J.D.L., Burnard, E.D., Davis, J.A. *Lancet*, 1957, i, 313.
3. Ministry of Health: Report of the Maternity Services Committee. H.M. Stationery Office, 1959.

HELP FOR THE WHEEZY

WHEEZING is commonly attributed to bronchospasm; but Farber and Wilson [1] point out that, even in young asthmatics, this is only one of its causes: mucosal swelling and the secretion of viscid mucus are others. They urge, therefore, that the advantages of aerosol therapy with adrenaline and isoprenaline should not be overlooked; for this mode of administration exploits the vasoconstrictor and antisecretory as well as the spasmolytic properties of these drugs. Moreover, since vasoconstriction limits absorption of the drugs into the general circulation, comparatively large quantities can be given without producing undesirable side-effects; and, since injections are avoided, long continued use of adrenaline becomes feasible. For the treatment of chronic bronchospasm by systemic administration, Farber and Wilson regard ephedrine as still the best of the sympatheticomimetic drugs, the more powerful action of isoprenaline being offset by its side-effects, especially palpitation. They are impressed by the dramatic relief often given by intravenous aminophylline in acute bronchospasm, but they hold that oral theophylline compounds have not much to offer, because of their tendency to irritate the stomach. Many physicians in this country, however, would endorse the finding of Gandevia et al.[2] that choline theophyllinate is well tolerated and reasonably effective. Agents which inhibit the action of acetylcholine have so far proved disappointing as bronchodilators, but Farber and Wilson report mildly encouraging results with methyl scopolamine nitrate and think that interest in this group of drugs is likely to increase. With regard to indirect bronchodilators, they rightly point to the importance of antibiotic therapy, drawing attention to the value of reducing the viscidity of sputum by hydration and other methods, and to the possible benefits of corticosteroid therapy and of desensitisation techniques.

Helpful though bronchodilators are, they can give only limited relief to the patient with emphysema, whose wheezing is partly due to mechanical factors.

1. Farber, S.M., Wilson, R.H.L. *Ann. intern. Med.* 1959, **50**, 1241.
2. Gandevia, B., Hume, K.M., Prime, F.J. *Lancet*, 1957, i, 956.

In emphysema the elasticity of the lungs is diminished, with the result that during expiration —especially forced expiration—the bronchi are compressed by the surrounding alveoli and air-flow is impeded. Increasing the bronchial intraluminal pressure during expiration helps to keep the bronchi patent, and some emphysematous subjects tend to do this spontaneously by means of "pursed-lip breathing". The use of an expiratory resistance mask serves the same purpose, but it demands extra work from the patient and on balance is rarely profitable. Farber and Wilson think that in chronic bronchitis and emphysema intermittent positive-pressure breathing (I.P.P.B.) is valuable because bronchial dilation in the inspiratory phase releases pent-up secretions and permits delivery of nebulised drugs to otherwise inaccessible parts of the lungs. They admit, however, that its value has not been established, and certainly it does not solve the crucial problem of obstruction to expiratory air-flow. Because expiration remains slow and may not be complete before the next inspiration, Opie et al.[3] warn us that I.P.P.B. may lead to a permanently raised intrathoracic pressure dangerous to a patient perhaps on the verge of circulatory collapse; and Nunn [4] has pointed out that application of a negative pressure in the expiratory phase may make matters even worse by increasing the tendency to bronchial collapse and air trapping. It is clear, therefore, that I.P.P.B. should be used only with considerable circumspection.

Abdominal binders, belts, and pneumoperitoneum have been used to raise the diaphragm in the hope of improving faulty thoracic mechanics, but these manœuvres have now largely been abandoned. There is no objective evidence that exercises for emphysematous and asthmatic patients enhance pulmonary function; but improved muscular coordination may reduce fatigue, and in children thoracic deformity can be prevented or corrected. Apart from the boon of antibiotics, there is no doubt that in the past few years the wheezy patient has benefited considerably from the cumulative effect of small therapeutic mercies, but he still awaits a windfall.

HEALTH IN THE POTTERY

THE latest report by H.M. Factory Inspectorate[1] contains an excellent description of those types of work in the pottery industry which carry a risk to health. But it is *Hamlet* with hardly any of the Prince because it contains so little information about the actual health of potters. Though the survey team included a doctor, no mention is made of medical examinations.

Despite great improvement of late years—notably in the substitution of alumina for flint in some processes, and in the control of exposure to lead—the factories observed seem to present the normal distribution of good, mediocre, and bad, the last including some which have apparently ignored the regulations. But the significance of shortcomings cannot be weighed satisfactorily without information about the past and present incidence of disease among the employees. For instance, the "potential risk of pneumoconiosis" reported by the team applies to many dusty industries. For the pottery industry statistics exist, and they could well have been included and discussed in the light of the team's comments on particular tasks and factories. Such environmental data concerning dusts as are given in the report are too few to be of much help in assessing the risk throughout the industry, or even in any one occupation, and the existence of abnormally dusty processes in which no dust-counts were made suggests a risk of pneumoconiosis which is actual rather than merely potential. As for the risk of lead poisoning, this appears to be well controlled, but the evidence presented on absorption of lead is not convincing, because the subjects chosen for blood examinations were taken from potteries with reasonably good standards, and because the examinations were confined to counts of punctate basophils under transmitted light. It is pretty generally agreed that isolated counts of stippled cells are misleading, because they vary from day to day or even from hour to hour. Dark-ground illumination is held to be the most effective.

The most striking part of the report, from the health standpoint, are some facts about atmospheric concentrations of fluorine compounds, ant it seems that an opportunity was missed of making clinica[1], radiographic, and biochemical examinations of workers so exposed. Information from such examinations, together with the environmental date, would have been of considerable interest, but by themselves the environmental data are of little significance. Similarly, though benzene and nitrobenzene were found to be used in the industry, no mention is made of any attempt to measure absorption of these substances. Was the absence of symptoms of poisoning regarded as enough?

The report is interesting and ably written, though rather in the style of an intelligent non-medical journalist. But we may question the claim in the foreword that it is "the only study of its kind which was ever been made of the practical conditions in the pottery industry, or, indeed of any other one industry in the country". This claim seems hard to justify when one recalls the surveys of health and working conditions in coalmines and foundries—to mention only two.

MATERNITY AN EMERGENCY

LAST year the London Emergency Bed Service was asked to arrange last-minute admission for 2591 nomal maternity cases, an increase of almost 900 on the previous year[1]. Most of these patients had been attending antenatal clinics which had been unable to book them a bed in advance, but had advised them to arrange emergency admission when labour started. The E.B.S. points out that this is a most unsatisfactory procedure, especially for the patient, who is often put to unnecessary worry and inconvenience. Moreover there have been many cases where the baby was born in the ambulance, or at home before a hospital could be found. This situation needed an urgent remedy last year, the report says, but this year the position is even more serious.

The E.B.S. had no difficulty in finding beds for acutely ill patients until mid-January. But a month later, when the effects of the influenza epidemic and the fog were at their greatest, there was grave doubt whether beds could be found for all who needed them. Furthermore, as others found[2], the situation was made even more serious by illness among the nurses of the hospitals. In the seven days ending Feb. 23, 2419 requests were received, only 100 fewer than in the worst period of the great fog in December, 1952.

The annual report emphasises that ideally the use of the medical referee, which relies on compulsion to obtain admission for an acutely ill patient, should not be necessary. At 11.6 % the proportion of patients admitted in this way is still too high, although it is better than in 1956, when over 15 % of patients were admitted by the direction of a medical referee.

3. Opie, L. H., Spalding J. M. K., Stott, F. D. *ibid.* 1959, **i**, 545
4. Nunn, J. F. *Brit. J. Anæsth.* 1958, **30**, 134.
1. Industrial Health—a Survey of the Pottery Industry in Stoke-on-Trent. A Report by H.M. Factory Inspectorate. H.M. Stationery Office, 1959. Pp. 78. 5s.

1. Report of the Emergency Bed Service for the year ending March 31, 1959. Pp. 12. King Edward's Hospital Fund for London, 28, London Bridge Street, London, S.E.1.
2. *Lancet*, 1939, **i**, 894.

Special Articles

OPERATION RIP VAN WINKLE *

EDWARD S. STERN
M.A., M.D., Cantab., M.R.C.P., D.P.M.
MEDICAL SUPERINTENDENT,
CENTRAL MENTAL HOSPITAL, HATTON, WARWICK

In 1955 we estimated that rehabilitation was worth attempting in a third of our chronic patients. Few of them had florid mental symptoms, and they occupied themselves fairly well; yet when they left the permissive atmosphere of the mental hospital they found the bustle of industry too much for them. Some were tried in the industrial rehabilitation unit of the Ministry of Labour, yet even there the pace was too hot.

INDUSTRIAL UNIT

With the object of inculcating the habit of regular normal work, and of toning the patients up so that they could manage it, we started a rehabilitation unit in which a small number did simple electrical dismantling, to recover their manual dexterity.

We had no premises for the purpose, but on Oct. 10 1955, we began with 3 women and 3 men each in two single bedrooms which we borrowed by day. To simulate a factory atmosphere they were put on time-sheets and paid 3*d*. an hour.

The scheme was so successful that expansion was soon required. Fortunately a corrugated-iron shed became available. As this was in a sorry state, the patients had to rehabilitate it before themselves being rehabilitated.

The rehabilitation unit soon became accepted by the staff as a normal and essential therapeutic tool, and by the patients as the path to discharge and independence, however long their illness might have lasted. Not only did they welcome transfer to the unit, but they spontaneously asked for it.

The maximum number we can treat at the same time is 18. Most need to stay three months in the unit. Patients over 60 are not usually accepted.

By the end of three years 65 men and 18 women had entered the industrial unit. Of these, 25 men and 4 women had been discharged to outside employment and homes, 10 men and women had not been rehabilitated, and a further 11 men and 2 women were considered unsatisfactory (including some withdrawn because of physical illness, 2 who died, and others who left hospital against our advice). 22 were still being rehabilitated, some of them at the Coventry industrial rehabilitation unit. Thus of 61 completed cases, 29 were successful. Results have steadily improved as we have gained experience, but so far we have only been learning the method, and have not attempted a statistical evaluation.

Relapse-rates have been low; but we think it important for the rehabilitated patient to obtain a job that is not too distant, so that we can help him if he should be in danger of breaking down again.

DOMESTIC UNIT

Industrial rehabilitation, excellent as it may be for the working man or woman, is of little help to the housewife. Since she is classed as "not gainfully employed", she is not an insured worker, and so the Ministry of Labour does not provide for her. We discovered, however, that two well-run units for the rehabilitation of the housewife and mother were in existence,—one sponsored by the Society of Friends, and the other in a prison. Having visited these, and having also obtained advice from the county concil's head domestic-science teacher, we set our patients and staff to repair another part of the old hut. We installed a second-hand solidfuel "Triplex" cooker and a rejected gas cooker and collected and polished up some old hospital furniture, to make a living-room kitchen, scullery, larder, bedroom, and sitting-room.

We opened fully on April 23, 1958, under a married part-time nurse who is also an efficient housewife. One look at the bright faces of the patients sent to the unit showed that is was a happy place and augured well for its future.

Besides the ordinary household chores, cooking is done for the ladies in the unit and for a few of the men in the industrial unit. Vegetables and flowers are grown, and fowls are kept on the adjoining ground. The nurse takes the patients shopping at the nearest town each Friday. All attend a cookery demonstration, given by the county instructress every Wednesday morning, which is also attended by other patients.

Unit hours are from 9.0 a.m. for a five-day week. The maximum number is 4 patients. 10 patients have been to the unit, 4 have been discharged to their homes, 3 are still in hospital, and 3 have not yet completed their time in the unit.

The popularity and promise of the domestic unit were such that a second one had to be opened at once in our non-statutory hospital, Leigh House, and after that a third. These are run on similar lines, but, as the unit at Leigh House deals with acute and mild cases only, all who go through it are discharged from hospital afterwards. Here again a striking feature has been the happiness of the patients. In a household unit melancholics soon become cheerful. Perhaps this is due the their return to a feminine role, for which the modern system of a centralised hospital commissariat does not fully allow.

COMMERCIAL UNIT

A need was felt for rehabilitation of the many women clerical workers. Here we met an unexpected difficulty, for we were told that we could not order typewriters because these were office supplies. After temporising with a borrowed machine, however, the unit was equipped with condemned office typewriters and a duplicator. We have thus gradually accumulated 4 serviceable typewriters, and consider ourselves fairly well equipped. Moreover, fortunately, our chaplain is expert at shorthand and typing, and kindly gives up much of his time to teaching the 4 ladies in the unit. He began the classes in June, 1958, and we opened the unit fully on Aug. 16.

Hours are from 9.30 a.m. to 3.45 p.m., with the usual lunch break, for a five-day week. 2 ladies have already left and taken up paid work outside. In the unit, instruction is combined with useful work for us all. Once again a striking feature is the happiness of the patients e.g., one of them who had previously drifted from one depressive mood swing to another has become stable since she entered the unit, and has gained a new sense of purpose.

HANDYMAN TRAINING

It was evident that our industrial unit had one great weakness — that it would be difficult to place some of our rehabilitees in employment if a trade recession came to the Midlands. But there is always a steady demand for handymen — i.e., domestic workers who can garden, clean the boots and the car, and do minor household repairs. So the cooperation of the works and utility departments of the hospital was obtained, and arrangements are being made for patients to work through them, with the idea that they should learn rather than be useful. We began on Jan. 12 by sending 3 men each to the painters' and carpenters' departments as pupils, and we have planned the extension of this to other activities.

Our need for a greenhouse was met by the staff and patients building one from the timber thrown away during the replacement of two old floors in the hospital. Three garden frames and a potting-shed have been

* Substance of a lecture given to senior staff at the Midland Nerve Hospital on March 4, 1959.

constructed similarly. Ground over buried hot-water mains has been ingeniously cultivated to make a forcing-bed.

DISCUSSION

Previous attemps at curing chronic patients often seem to have failed because it was not understood that the patient is principally sufferin from the effects of long institutionalisation and unemployment. He has become settled in his little world, and he dreads the greater one which has rejected him and in which he has failed. This world too—like that of Rip van Winkle—has changed since he became ill.

Any success our units have had and the smoothness with which they have run are perhaps due to several causes. We did not begin the scheme until more than four-fifths of all our patients were occupied, nor until we had almost completed unlocking all our doors, as described in Operation Sesame 1. Patients were not transferred to the units until socially recovered. No immediate incentive, such as high wages, was offered, but it was made clear to the patient that he was sent to the unit for rehabilitation with a view to discharge. The unit was not really a "factory in a hospital" for there was little emphasis on output (although incidentally this has been satisfactory): all our efforts have been concentrated on the patient and his recovery. It is only too easy for dull repetitive work to encourage chronicity, and for so-called therapy to produce a dull industrialised psychotic robot instead of a recovery.

Furthermore the scheme was not begun until after a careful study of the Ministry of Labour's industrial rehabilitation units in various parts of the country, so that I was able to avail myself of their vast experience. Finally I took the staff into my confidence early, and it is their enthusiasm and help that has sustained the scheme so well.

Further advances are planned. The Nuffield Provincial Hospitals Trust has generously given us a grant so that we can build a proper domestic unit. The regional hospital board has recognised our scheme and are to spend about £ 35,000 on an industrial rehabilitation unit which we are now planning; and this will have a gymnasium, which we lack at present, and proper facilities for instruction, accommodation of staff, and so on. We plan that at least the domestic rehabilitees shall be resident in their unit, for they find that return to the wards at night tends to set them back again. We hope to have a hostel for working patients, as a halfway house, outside the mental hospital. The unit needs its own psychiatric social worker, psychologist, and clerical staff, to put the work on a more scientific basis.

1. Stern, E.S., *Lancet*, 1958, i, 557.

Letters to the Editor

THE HYPERVENTILATION SYNDROME

SIR,—Your comment of June 27 on hyperventilation and its relation to other disease is timely in this hot weather. In the Middle East this clinical syndrome was often seen in young men and children. Periodic breathing, cyanosis, and carpopedal spasms were common, and I have seen a case of epileptiform convulsions apparently the result of cerebral vasoconstriction due to a low pCO_2.

The usual sequence was acute tonsillitis, high fever, and hyperventilation aggravated by the intense heat. Even incipient alkalotic tetany can be very startling, since the premonitary symptoms (tingling in the fingers with pseudoparalysis of the hands and feet) are suggestive of poliomyelitis, especially when there is anxiety about this disease.

One really difficult differential diagnosis in the Middle East was from bacillary dysentery. Acute abdominal colic can be present in both and it may precede the diarrhoea of dysentery. One case, in this country, appeared to be directly attributable to anxiety induced by contact with a case of encephalitis. Another case, in which I was lucky enough to see an attack, had been investigated by lumbar puncture and electroencephalography two years previously for "fits". This man's brother had been refused for National Service because of "blackouts and shaking attacks".

The conventional treatment by isolation from bystanders, reassurance, sedation, intravenous calcium gluconate, and breathing into a bag or a mixture of oxygen and 5% carbon dioxide gives dramatic relief from a superficially very alarming syndrome. The important things to note are (1) it is relatively common; (2) it may stimulate other diseases or coexist; (3) a small proportion of cases are dangerous and progress to coma and death; and (4) it gives rise to panic among onlookers.

D. M. DAVIES.

St George's Hospital,
London, S.W.1.

BREAST CANCER

SIR,—Mr. Donaldson (July 4) seems to think that my letter of June 20 was about the treatment of breast cancer, but of course it was only about the way in which the results should be assessed.

ROBERT PLATT.

Department of Medicine,
The Royal Infirmary,
Manchester 13.

BLANKETS AND INFECTION

SIR,—In a paper which you published on Oct. 4, 1958, Schwabacher, Salsbury, and Fincham reported that the number of airborne bacteria in a hospital ward was reduced when woollen blankets were replaced by cotton blankets. Presumably the wool blankets were of the normal, fluffy, "raised" type, while the cotton blankets were stated to be of cellular construction. No evidence was submitted to support the view that the change in fibre composition, rather than the change in fabric structure from raised to cellular, was the cause of the observed results.

When further information concerning the structure of the woollen blankets was sought, Dr. Schwabacher kindly provided the name of the supplier to whom inquiries could be addressed. From this source, however, the astonishing information was received that the so-called "wool" blankets used in the investigation contained 47 % man-made fibre, 35 % cotton, and only 18 % wool. To obtain an independent check of the blanket composition a sample was taken from the same batch of blankets as that used in the Peace Memorial Hospital tests and this was submitted for analysis to the Textile College of the Gordon Institute of Technology at Geelong in Victoria, Australia. Their report states that the warp is cotton yarn and the weft a blended yarn containing approximately 50 % bright viscose, 25 % delustred viscose, and 25 % wool. The wool represents 10.4 % of the total blanket weight. The blanket was of the raised variety.

Thus apparently wool blankets were not in fact used in the Peace Memorial Hospital trials, and the conclusions drawn by the authors of the paper are therefore unjustified.

F. G. LENNOX

C.S.I.R.O. Division of Protein Chemistry,
Parkville,
Victoria,
Australia.

⁂ We have shown this letter to Dr. Schwabacher, who writes: "Mr. Lennox's letter shows that blankets sold in England as 'woollen' are in fact not pure wool. However, 'woollen blankets' used in hospitals produce *fluff* which has been proved to be a major factor in cross-infection."—ED. L.

MEDULLARY GONADAL DYSGENESIS

SIR,—The paper by Dr. Stewart in your issue of June 6 seems to raise once again the question of whether certain investigations are justifiable.

Two mentally subnormal children were subjected to testicular biopsy so that a diagnosis of Klinefelter's syndrome could be confirmed. Presumably these biopsies were carried out under a general anæsthetic. Presumably also there was a certain amount of postoperative pain. No general anæsthetic is entirely free from risk. The question therefore arises, what benefit was hoped for for those two patients?

Could Dr. Stewart answer the following questions:

What treatment, if any, resulted from the investigation?

What guidance in treatment was hoped for from the testicular biopsies?

Supposing treatment to be possible to what end could it be directed. Surely not to increasing the poteney or the fertility of those mentally deficient youths?

It is important to ask these questions because the diagnosis in cases such as this is of the greatest scientific interest, and the investigators' desire for all possible data is, therefore, very great. Nevertheless, dangerous diagnostic procedures can only be justified if there is a reasonable hope that accurate diagnosis will be a guide to treatment. If this is not so there would seem to be no reason why these children should not also have been submitted to biopsy of the adrenal cortex or even of the pituitary body.

Halstead, Essex. R. E. W. FISHER.

Dr. Fisher's letter has been shown to Dr. Stewart whose reply follows.—ED. L.

SIR,—Dr. Fisher's questions could well be discussed at length in a Dialogue of Today, but at present brevity seems desirable. Diagnosis of chromatin-positive Klinefelter's syndrome is easy, but it is hoped to do more for our patients than this. Biopsy is necessary to assess fertility, and this is of direct importance to each individual patient. Biopsy is of value in increasing scentific knowledge, which it is hoped will ultimately benefit many patients with hypogonadism, and with oligophrenia.

Management includes advice to the patient, and treatment to alleviate the stigmata of the syndrome. In neither of the two patients with oligophrenia is the impairment severe. The elder has already left school and is usefully and happily employed as a waiter in a city restaurant. Their oligophrenia is not a contraindication to marriage, and there is no reason why they should not have carefully supervised androgen therapy to increase their potency if this is necessary. The question of possible fertility raises wider issues, but mild oligophrenia alone can hardly be accepted as sufficient reason for denying these patients any possible therapy.

I strongly deprecate the eugenic implications of Dr. Fisher's suggestion, and wonder if he would be prepared to set a level of I.Q. below which patients should be deprived of treatment for this or other conditions.

Glasgow Royal Infirmary. JOHN. S. S. STEWART.

MORAL OBLIGATIONS

SIR,—Dr. Easton's letter in your issue of June 27 shows how desirable the Socratic method is for the exposure of error and misunderstanding. "Eryximachus...", he writes, "stressed that doctors should not necessarily be entitled to compensation for loss of office". Who would think that what I said was:

"It is a sound principle that those who have been wronged or suffered through no fault of their own should, as far as possible, be compensated, and the appropriate compensation might be a pension for those for whom no other suitable work can be found"?

ERYXIMACHUS.

BOXING

SIR,—Mr. Ian M. Leslie's reply (June 20) to your admirable editorial of June 6 calls for a rebuttal. To say that boxing is "a character-forming sport" and that it engenders "courage, self-reliance, physical fitness, and chivalry" is an assertion not based on evidence. There seem, in fact, to be good reasons to justify qualified statements to the contrary.

The allegation that your argument against boxing is open to criticism because of "the reliance it places on American data" is incorrect. Actually, with typical British reticence, you have failed to stress that so many relevant clinical studies of boxing were conducted in the British Empire[1], among them the magnificent case analyses by Macdonald Critchley of the National Hospital, Queen Square, and of W. V. Bremner[3] of the Royal Canadian Naval Medical Service, as well as the researches by my colleagues and myself undertaken during the years I spent in South Africa[4]. As regards Mr. Leslie's reference to the attempt by Kaplan and Browder[5] to exculpate boxing, the electroencephalographic investigations by Larsson et al.[6] of Stockholm, and of Pampus et al.[7] of Bonn, have proved beyond doubt that whatever the merits of these writers' material may be, their conclusions do not apply to any but the subjects with whom they deal. I cannot see the logic of Mr. Leslie's argument when he rejects the validity of American medical literature on boxing but quotes as his lone reference the one U.S.A. publication whose data and conclusions could not be corroborated anywhere.

Mr. Leslie points out that the head is not the only "recommended target" in boxing implying, I take it, that the rest of the body is not vulnerable to blows. A sufficient number of medical reports are on record (of which a few are quoted below[8]) proving the fallacy of such a pronouncement. The incidence of this group of traumata would be still greater were it not for the remarkable taboo, fortified by strict rules, which serve to protect the lower half of the boxer's body—a taboo not invoked in respect of head and trunk.

No categorical difference exists between the dangers of amateur and professional pugilism. Most the clinical and pathological states which were encountered by the late Eric Guttmann and myself after we started our study of boxing as a medical problem involved amateurs[9]. In its reaction to injury living tissue is not influenced by the monetary arrangements between the boxers' managers.

University of Kentucky, Lexington, Kentucky. ERNST JOKL.

IMMUNOLOGICAL ASPECTS OF INFERTILITY

SIR,—In your annotation of June 27 you say that in 16 of my 21 azoospermic patients with sperm agglutinins in the serum the efferent ducts were blocked by the agglutinated masses of spermatozoa. Actually, in these 16 cases the efferent ducts were occluded because of old inflammations (gonorrhœa, tuberculosis), congenital absence of the vasa deferentia, or surgical accidents during herniorrhaphy in childhood.

This suggests that occlusion (or absence) of the

1. Will, G. W. *J. R. Army med. Corps*, 1939, **72**, 389; Fallon, *Brit. J. Surg.*, 1940, **28**, 39; Parsons-Smith, G., Williams, *Brit. med. J.* 1949, i, 10; McAlpine, D. *Proc. R. Soc.* 1949, **42**, 792; Doggart, J.H. *Arch. Ophth.* 1955, **54**, Muskat, D. A. *Med. Proc. (S. Afr.)*, 1958, **4**, 172.
2. Critchley, M. *in* Hommage à Clovis Vincent (Paris, 19 and *Brit. med. J.*, 1957, i, 357.
3. Bremner, W. V. *J. R. nav. med. Serv.* 1950, **36**, 202.
4. Jokl, E. Medical Aspects of Boxing. Pretoria 1941.
5. Kaplan, A. H., Browder, J. *J. Amer. med. Ass.* 1954, 1138.
6. Larsson, L. E., Melin, K. A., Nordström-Öhrberg, G., verskiöd, B. P., Öhrberg, K. *Acta psychiat. Kbh.* suppl. 95.
7. Pampus, F., Grote, W. *Arch. Psychiat. Nervenkr.* **194**, 152.
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deferens or epididymis is the *cause* of the presence of the sperm antibodies in the serum—not the result, as you have concluded. Moreover this is readily explained, since we know that in patients with occlusion—whatever the cause—one often finds spermatostasis in the epididymis with infiltration of spermatoza in the interstitium. I have no arguments for the assumption that sperm agglutination precedes tissus penetration as you have stated.

In patients without azoospermia, autoagglutination of the sperm is often found, attributable to the presence of sperm agglutinin in the seminal plasma as well as in the serum. In several of these cases a one-sided occlusion has been found.

Centraal Laboratorium van de Bloedtransfusiedienst van het Nederlandsche Roode Kruis, Amsterdam. PH. RUMKE.

EMERGENCIES AT SEA

SIR,—I am afraid the paragraph about mental illness in my article of June 20 was very badly phrased. I can assure Dr. Stafford-Clark that no ship's captain sends urgently for a doctor in these cases unless he thinks he has a dangerous lunatic aboard. As far as I can remember, all my eleven cases had to be certified and the main problem was how to get them ashore without serious injuries to the crew or the patient, and without capsizing our small boat!

Unfortunately I did not refer to the many cases of minor neurosis treated (I hope) with kindness and calmness. I had in mind only cases of major lunacy requiring fortitude and restraint.

Walmer, Kent. JAMES S. HALL.

MURUTS OF NORTH BORNEO

SIR,—May I correct a statement in your report (May 30, p. 1156) of my lecture on the Muruts and their declining population? This report states that "Inquiry... disclosed that abortion was commonly practised in nulliparous women, especially before marriage". Although inquiry revealed that many nulliparous women have had abortions, no evidence could be obtained that these abortions had been induced. This is very different from the situation on some Pacific islands, where induced abortion has been thought to be a major cause of depopulation.

Department of Social Medicine and Public Health, University of Malaya in Singapore. IVAN POLUNIN.

DANGEROUS PLASTIC BAGS

SIR,—My nephew, aged 3½ years, would have been suffocated by a polyethylene bag, which he had pulled over his head, but for the providential arrival of his mother who found him lying on the floor, blue in the face. The bag had contained potatoes and had come from the greengrocers. It had two air holes in it; in spite of this, because of the softness of the material, it had been sucked into mouth and nostrils on inspiration, completely obstructing them. These bags are lethal.

London, S.W.1. PATRICK STACK.

THE MEDICAL DIRECTORY

The Editor of *The Medical Directory* writes: "The annual schedule, upon the return of which the accuracy of *The Medical Directory* mainly depends, has recently been posted to all traceable members of the medical profession, and its speedy return would be appreciated.

"A number of schedules, sent to a doctor's last known address, have been returned through the post-office marked 'Gone Away'. Although in such cases every effort is made to find a new address, I should like to remind practitioners that if these efforts are not successful before the book goes to press, the names in question will be omitted from the next edition.

"Any doctor who has not received the annual schedule, or who has mislaid it and would like a duplicate, is requested to communicate as soon as possible with The Editor, *The Medical Directory*, 104, Gloucester Place, London, W.1. The full name and year of qualification of the doctor should be sent for identification, please."

In England Now

A Running Commentary by Peripatetic Correspondents.

The drastic but unavoidable reduction in the size of *The Lancet* over the past few weeks, although no doubt regretted by some, has been of inestimable benefit to others. I have at last been able to *catch up on my reading*. Florey and Chain have been working on the purification of an extract from a mould which, it seems, has considerable antibacterial activity and might well find some useful clinical application. I am hoping, rather selfishly, that the printing difficulties will continue long enough for me to follow-up this remarkable development.

* * *

Much has been written about the doctor-patient relationship—but practically nothing about the doctor's wife-patient relationship. Yet where a wife acts as receptionist, she is able and must often be tempted by love and concern, to withold the doctor from the patient. How much better to delay the call, lest the doctor put up his feet, and enjoy his meal. On the other hand, like the thin man in every fat man, there is a patient or patient's mother in every G.P's wife. I am particularly vulnerable to babies "who won't stop crying" and children with "terrible ear-aches". There he is, ready to enjoy dinner. Shall I pretend that the meal is not ready. "The meat didn't come till late—the cooker misbehaved—there's just time to rush out." My apostolic function knows no bounds.

Close as I am to my husband, my own *furor therapeuticus* makes me even closer to the patient. Often I have had to disentangle myself from the pastry, rush to the phone, festooning the handpiece with rough puff. But the strain of the phone is as nothing to the threat of the front-door bell. The anonymity of the phone ceases. One knows their faces, has talked to them repeatedly, met at the grocer's. Though the name may never have been mentioned, you know all about their bowels and begonias. How can you say "And what is your name?" When they say "You know me", they are daring me not to know. And if I don't know them, and talk about them, how can doctor care enough for them to make them better?

Another complication is that the doctor's wife must be something of a lady; busy, of course, but not lighting or laying fires, or cleaning steps: something genteel—planning menus, sorting the rare china, or even polishing the silver if delicate and antique enough. The only possible time for dirty chores is before dawn or after midnight. Failing that, holding the washing in a large basket, well in front of the face, is an effective disguise.

But your big moment comes when the patient confides in you, with the strict but coy injunction "not to tell doctor". You are, of course, expected to tell doctor. A more overt way of "not telling doctor" is when the patients drop in after the car has left and describe their (or friends) symptoms to you. These, I am asked to mention to doctor, and later "pass back" his advice. In re-telling a conversation a little judicious editing is sometimes domestically useful. For instance, "Mr. X's muscle was strained while turning out the shed" or "laying the linoleum for his wife." But this brings us to the doctor-wife/patient relationship.

* * *

I welcomed him to the surgery, for the last time that we had met he was exsanguinated and on his way to hospital. "Took me a bit longer to get on my feet, than I expected, Doc" he grudgingly admitted. "Nearly froze to death when they gave me that there blood-transfusion, fair set me back with a bad attack of the shivers. It had the doctors puzzled for a while, they couldn't make head nor tail of it". He paused as a look of righteous indignation spread slowly over his face—"But I got to the bottom of it when I was up and about. One day, I went to get myself a drink of milk from the ward 'fridge and—would you believe it—there, inside, were bottles and bottles of ice-cold blood"

* * *

Imprimé en France.—Leaping pale and shaken from the motor-bicycle at the Arc de Triomphe, the Group-Captain raced towards the control point. "Quarante-trois minutes!" But cheers died as he sped past, ignoring the time-clock, to a figure at a desk on the other side of the Champs-Elysées. They roared out again as the loudspeaker announced: "Le record n'est pas battu, mais les articles *Lancet sont* délivrés de bonne heure."

Obituary

OWEN LAMBERT VAUGHAN SIMPKINSON DE WESSELOW

D.M. Oxon., F.R.C.P.

Prof. O. L. V. de Wesselow, consulting physician to St. Thomas's Hospital, ,who died on July 6 at the age of 75, was ahead of his time in his biochemical approach to medicine. He belonged to the era of personal research before the days of big departments with many assistants and technicians; but his contributions, especially to our knowiege of diseases of the kidneys and of hypertension, were important and fruitful.

He was the elder son of the Rev. C. H. Simpkinson (who later changed his name by deed poll to de Wesselow). He was educated at Haileybury and at Corpus Christi College, Oxford, taking a first class in the final honour school of physiology in 1905. From St. Thomas's Hospital he qualified in 1908. After postgraduate study in Paris he began chemical research work under Sir Charles Martin's direction at the Lister Institute, but this was interrupted in 1914 when de Wesselow joined the R.A.M.C. He served in the trenches with the Worcester Regiment, being wounded and mentioned in despatches; later he investigated trench nephritis and trench fever in the laboratory at Etaples. He had been offered the appointment of assistant physician at St. Thomas's but he preferred to remain on active service.

By the end of the war his work on nephritis and blood diseases had already established his reputation as a biochemist, and on demobilisation he was appointed assistant physician to St. Thomas's Hospital and physician to the General Lying-in Hospital, York Road. Here he studied the toxaemias of pregnancy publishing a book on the subject with J. M. Wyatt. He also wrote a monograph on the *Clinical Chemistry of the Blood*. In due course he was appointed director of the medical unit at St. Thomas's Hospital and professor of medicine in the University of London and physician to the Hospital. He served the Ministry of Health as consulting physician to the Board of Control, as a member of the Departmental Committee on Maternal Mortality and as a member of the Nutrition Committee of 1936-39. He was also Croonian lecturer to the Royal College of Physicians in 1934, a member of the British Pharmacopœia Commission, and for many years editor of the *Quarterly Journal of Medicine*.

On the outbreak of the late war he took the post of sector hospital officer (Surrey) in the E.M.S., moving from his house at Epsom to Kingston-on-Thames. He also continued to visit and teach at Hidestile, Godalming, to which St. Thomas's Hospital had largely evacuated. After he retired in 1948 he lived at Goodworth Clatford, near Andover, and later at Winchester where he took an active part in local affairs and in the preservation of ancient buildings. He was a keen entomologist, an authority on military history, and interested in archaeology, and he collected first editions and old silver; but gardening was his chief pursuit and he had the proverbial green fingers.

H.K.G. writes:

"My first contact with de Wesselow was as a student in 1925, when he was assistant physician to M. A. Cassidy. He was then clearly a pioneer in clinical biochemistry: his rounds always had such a flavour, and his first publication had been with H. MacLean on the original test of renal function, the urea concentration test. He was a particularly keen undergraduate teacher and his rounds were a model of clarity. As a chief he was easygoing, and he allowed those whom he chose as his juniors to get on with their own work in their own way. In character he was a true-blue conservative, intolerant of humbug, pomposity, and vested interests. Even in consulting practice, he never owned a car; he was a keen mountaineer, a member of the Alpine Club; he was interested in rose growing; and he was an enthusiastic collector of butterflies, chalk blues being his great speciality. He was robust in personality and physique and at 75 his intellect was active and clear."

In 1918 de Wesselow married the youngest daughter of Sir Charles Craufurd, by whom he had one son. She died in 1948. In 1952 he married Mrs. Barbara Barratt, daughter of Lieut.-Commander Chamberlain, and she survives him.

RUSCOE CLARKE

M.B.E., M.B. Lond., F.R.C.S.

Mr. Ruscoe Clarke, who died on July 10 at the age of 51, had been senior consultant surgeon at the Birmingham Accident Hospital since 1947 and honorary secretary of the Institute of Accident Surgery since 1951. He was proud of the work of the Hospital in 1931 and worked there till 1936. He then as a centre of traumatic surgery rests on his work and his example. In the surgical world he was best known for his studies on post-traumatic hæmorrhage and shock, the use of blood-transfusion, and the surgery of internal injuries.

Educated at Haileybury, he qualified from Guy's Hospital in 1931 and worked theve till 1936. He then spent two years with a Leverhulme travelling fellowship in the United States, mainly at the Peter Bent Brigham Hospital and the Harvard Medical School, gaining experience in urology. In 1938 he returned to London, and, after holding surgical posts at Mile End and other hospitals, was appointed in 1940 first assistant in surgery to the British Postgraduate Medical School at Hammersmith Hospital, where his chief responsibility was in fractures and orthopaedic surgery during the strenuous years of the London blitz. There he came under the influence of Prof. A. K. Henry, who, he always said, was his surgical mentor. He joined the R.A.M.C. in 1942, and served throughout the North African and Italian campaigns. He reached the rank of lieut.-colonel and he was appointed M.B.E. in 1943 for his work during the Tunisian campaign.

In Birmingham, he applied his experience of war surgery to the problems of civilian accidents; he was a pioneer in the use of large transfusions and he advocated early and comprehensive surgery for the severely injured. His surgical skill was considerable and his speed remarkable; his judgment was sound and his leadership of a surgical team was by example as well as teaching. As an investigator his interests were directed to the body's reactions to injury. He was awarded the Arris and Gale lectureship at the Royal College of Surgeons in 1953, and through the Institute of Accident Surgery he had of late years taken an ever-growing part in the postgraduate teaching of traumatic surgery. The book on *Modern Trends in Accident Surgery and Medicine* in which he was part-editor and a major contributor was completed before his death, but he was not able to write the book he had planned on the teaching of first-aid to the injured, a subject on which be had thought, written, and lectured extensively.

A colleague writes:

"No surgical or human problem was too big or too small for Ruscoe Clarke if life or limb was to be saved or a patient made happier. His was a restless personality and he was endowed with immense energy which he always directed towards constructive ends. He was a good listener as well as a good advocate, and humble in his approach. He was always prepared to discuss pros and cons of individual cases, medical theory and practice, history, politics, and a host of other topics.

"An intellectual of broad vision, high principle, integrity, and courage, he combined theory and practice and he devoted a great amount of time and activity to the general interests of British medicine and the British people; this was directed, inter alia, towards a fuller and more comprehensive National Health Service, and, in recent years, to the abolition of nuclear weapons and testing, and towards the preservation of world peace. He was active in the work of the Socialist Medical Association, was a leading member of the British Peace Committee, and at one time was on the executive committee of the Association of Scientific Workers. An active Socialist, an educated Marxist, he was intensely proud of his membership of the Communist Party."

Mr. Ruscoe Clarke leaves a widow, two sons, and a daughter.